XXX. *On electrical and magnetic rotations. By* CHARLES BABBAGE, *Esq. F.R.S. &c. &c. Communicated May* 29, 1826.

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THE known fact, that artificial magnets receive additional increments to their power by slowly adding weight to the load they support, combined with the circumstance, that pieces of iron or steel exposed to the influence of a magnet, neither acquire nor lose their magnetism instantaneously, led me to explain Mr. BARLOW's experiments "On the temporary magnetic effect induced in iron bodies by rotation,"* without having recourse to any new property of matter, and to apply the same consideration of the time of acquiring and losing magnetic virtue to the curious experiments of M. ARAGO. These views are stated in a paper printed in the Phil. Trans. for 1825, which contains the observations that occurred to Mr. HERSCHEL and myself, in repeating and varying those experiments.

As the explanation there given is intimately connected with the experiments detailed in the present paper, and as they were undertaken with a view of giving additional evidence to the principle to which I have alluded, I shall here briefly re-state the manner in which time influences the results of certain magnetic phenomena, whose production depends on the application of motion to some part of the apparatus employed.

* Phil. Trans. 1825.

Fig. 1.

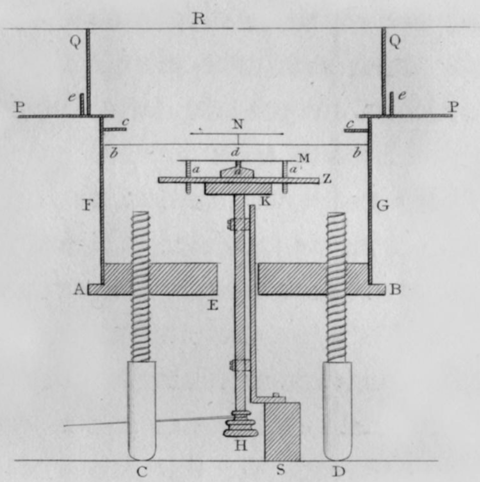


Fig. 2.

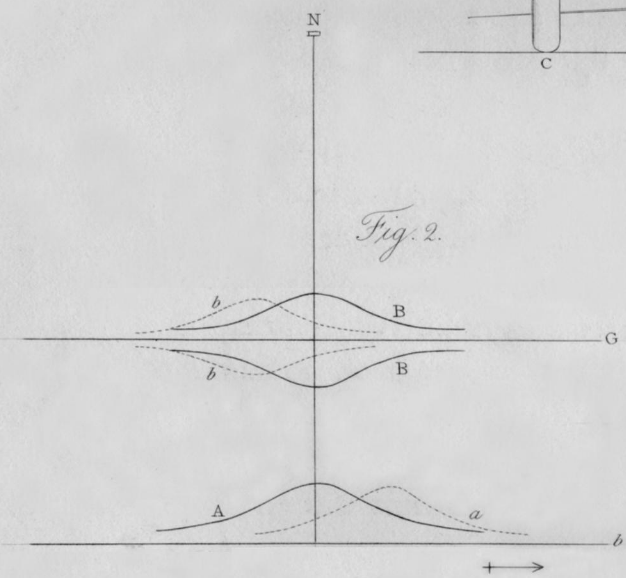


Fig. 3.

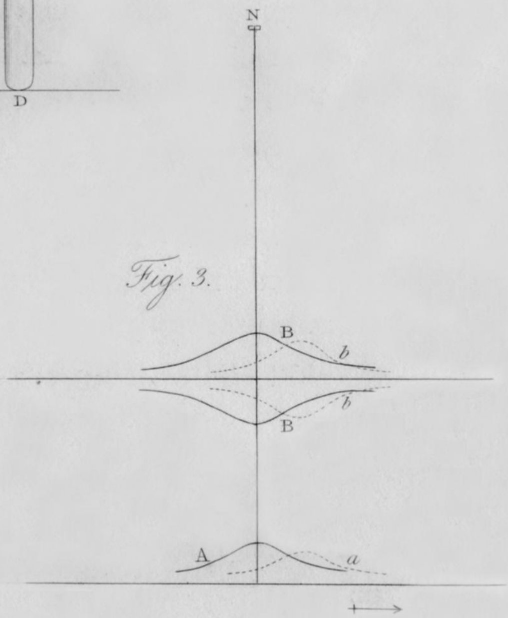


Fig. 4.

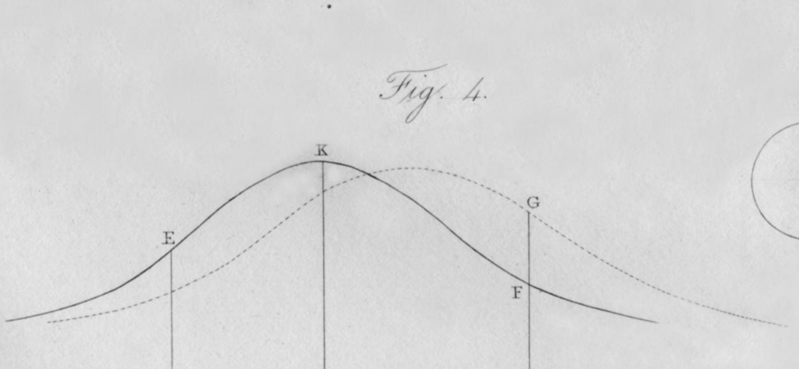
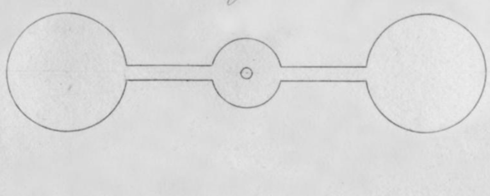


Fig. 5.



If N represent the north pole of a magnet, Plate XIX, fig. 4, placed above an indefinite plate AB, of iron or any substance susceptible of magnetism, then the point S immediately under N will become a south pole from induced magnetism: but the two poles will attract each other, and if they are prevented from moving directly towards each other, motion cannot arise in any other direction. The point S immediately under N will possess the strongest south polarity, but other adjacent points will also acquire the same property in a smaller degree: if an ordinate SK be erected at S, and if at every other point in the line AB other ordinates be raised, each proportional to the intensity of the induced magnetism at that point, a curve EKF will be formed, which will represent the magnetic force of the plate, and at equal distances CS, SD, on each side of the point S the ordinates EC and FD will be equal.

If the plate is now supposed to be moved in the direction AB, and if the time in which it acquires or gives up any portion of magnetism be not instantaneous, then the ordinates which represent the magnetic intensities at equal distances from the middle point S will not be equal: for the point D has, in consequence of the motion of the plate, just passed through a point S, considerably nearer to the source of magnetism, at which point its magnetic intensity was expressed by SK. As it is supposed not to part immediately with its acquired magnetism, some portion will remain when it reaches the point D: therefore the ordinate DG, at this point, will be greater than it is when the plate is at rest; consequently the curve EKG, expressing the magnetic force when the plate is in motion, will, on the side towards

which the motion is directed, be exterior to that which denotes the same force when the plate is at rest. But in the case of the plate being in motion, the force which is measured by the area of the curve is greater on one side than on the other; there will therefore be a small tendency in the plate to draw the point N towards that side to which it is moving. The points which have not passed vertically under the pole N, will in reality be a little less magnetised than they would be if they were at rest, because when they reach any particular place, they may not be exposed to the magnetic influence for a time sufficient for them to acquire the full degree of magnetism which they would obtain, if they remained longer in that place. If the moving plate be circular, and if the magnet be suspended in a balance of torsion, this extremely small tendency of each part to drag the pole N in the direction of its motion being repeatedly applied by each succeeding particle, and during many revolutions, may at last produce motion in the magnets above, even though the attractive force of the revolving substance be so exceedingly feeble as to be insensible to other methods.

In this analysis of the action of a magnet on a moving body susceptible of magnetism, it is to be observed, that no part of the effect is attributed to the velocity with which the agent* immediately producing the attraction traverses from the former to the latter: the nature of the action is not

* It is difficult to write on any physical subject without employing words which are more or less connected with the various hypotheses prevailing at the time. In the present Paper I have no intention of giving any opinion on the theories at present received, but merely propose stating facts that I have noticed, and the reasonings to which they appear to me to lead.

therefore analogous to that which produces the aberration of light.

The essential circumstance in producing the rotation of the suspended magnet is, that the substance revolving below it shall acquire and lose its magnetism in a finite time, and not instantly. It appeared, therefore, that if any other attractive or repulsive property exists in matter which is capable of communication to other matter situated near it; and if such communication require time for its performance; and if, when it is communicated, the force is not instantly annihilated on removing the cause, then such a property of matter may be substituted in the preceding reasoning instead of magnetism, the same deductions may be made, and similar consequences may be expected to result. On reviewing the known properties of matter, electricity seemed to be the only one which satisfied the requisite conditions: imperfect conductors placed in the neighbourhood of excited bodies do not immediately attain a state of electric equilibrium; and when they have arrived at that state which is due to such a position, on the removal of the excited body they do not instantly return to their former state. I therefore made the following experiments.

Experiment 1.

A needle was made of thin sheet brass, of the form, fig. 5, having two circular ends 1.25 inches in diameter, and the distance of their centres 3.1 inch; it had its edges coated with sealing-wax. Another similar needle was made, about one-third larger. One of these needles was suspended from a filament of silk, of several feet in length, at the distance of .25 inch above a circular glass plate six inches

in diameter, which was fixed by sealing-wax on a disc of box wood 2.5 inches in diameter: this latter was screwed upon a wooden axis. No electricity was communicated either to the needle or the glass disc, except that which resulted from cementing the latter by sealing-wax to the box wood on which it was fastened, and perhaps the coating of wax on the edges of the needle might have given it a very slight charge. The glass disc was now made to revolve at about 38 revolutions in a minute: there was a small motion of the needle in the same direction.

Experiment 2.

The same apparatus being employed, a large stick of common sealing-wax was excited and applied two or three times to the needle, in order to charge it. As soon as it was quite at rest, the glass disc was made to revolve more slowly than before, when the needle began to deviate in the direction of rotation, and followed the glass. Some doubt being expressed by a friend who was present as to the influence of the air set in motion by the apparatus, the electric charge of the needle was increased, and the velocity of rotation was diminished: the needle deviated as before, but much more rapidly. To remove some doubt as to the influence of any twist in the silk; whilst the needle was revolving in one direction, the rotation of the glass disc was reversed; after a short time the motion of the needle was destroyed, and it returned in the contrary direction.

For the purpose of ascertaining still more satisfactorily the effect which might be ascribed to the influence of air, the following experiment was made.

Experiment 3.

The apparatus was left many hours, in order that the electricity of its parts might, as far as possible, be dissipated. On returning to it, the glass disc was made to revolve with very great rapidity: not the slightest sensible effect was produced on the needle.

Experiment 4.

The needle was now electrified, and the same velocity being given to the glass plate, and all the other circumstances being the same,—the needle followed slowly. On trying various degrees of velocity, it was observed, that the effect on the needle was greatest when the motion of the glass plate was slow. A velocity of about five turns in a minute produced a considerable effect.

Experiment 5.

A stick of excited sealing-wax was substituted for the brass needle, and placed at the distance of about half an inch above the glass disc. On giving a slow motion to the glass plate, the wax needle followed in the same direction, and continued to move through many revolutions, notwithstanding the torsion of the silk; but on giving the glass a very rapid rotation, the sealing-wax needle remained nearly immoveable.

Experiment 6.

A new apparatus was constructed, which is represented in fig. 1. It consisted of a circular piece of wood A B, 9 inches diameter, having a hole of 1.5 inches in the centre,

supported by three legs, two of which only, C and D, are represented in the figure; these, by screwng into it, served to adjust its height. A cylinder of thick millboard F G, 5 inches high, was glued together, and was fastened to the board A B. In two very small recesses in its upper part rested a narrow bridge of glass or of wood *b b*, for the purpose of supporting a needle. A circular ring of pasteboard *c c*, was fixed near the top, which supported an engraved circle divided into degrees. A square piece of millboard P P, having a circular opening rather less than the diameter of the cylinder, rested on the top of it. Upon this was placed another millboard cylinder Q Q, of rather larger diameter, and $2\frac{3}{4}$ inches high. Another cylinder was fitted to the outside of this, in order to admit of fixing screens of muslin and various substances to the under side of the upper cylinder.

Through the aperture in the centre of the wooden plate A B, a steel axis working in a brass frame passed up into the cylinder. The frame was supported by being screwed to a block of wood at S. On the lower end of this axis a pulley H, was fixed, to which motion was given by connecting it with a common jack. On the upper end of this axis a thin brass chuck K 2 inches in diameter, was fixed, to which was attached a wooden disc Z, 5 inches in diameter. Three wooden screws *a a a*, rose from this plate, and supported a disc M, of any substance on which experiments were to be made.

A needle made of pasteboard covered with sealing-wax, and having an agate cap in its centre, was supported on the point of a steel sewing needle fixed in a wooden bridge 05 inch square. A plate of glass revolved below. The

following were the times in which different revolutions were performed.

When the glass disc made $26\frac{1}{2}$ turns per minute.			When the glass disc made $19\frac{7}{8}$ turns per minute.		
Number of Revolutions.	Time.	Difference.	Number of Revolutions.	Time.	Difference.
	′ ″	″		′ ″	″
0	21 0	49	0	34 0	52
·25	21 49	26	·25	34 52	28
·5	22 15	15	·50	35 20	17
·75	22 30	15	·75	35 37	15
1	22 45	42	1	35 52	45
2	23 27	35	2	36 50	43
3	24 2	35	3	37 35	40
4	24 37	33	4	38 18	39
5	25 10	32	5	38 58	38
6	25 42	32	6	39 37	40
7	26 12	29	7	40 15	40
8	26 44	38	8	40 55	43
9	27 16	30	9	41 35	41
10	27 45		10	42 18	

In the latter of these experiments, at the seventh revolution, the passing of a carriage may have slightly affected the remaining revolutions.

Experiment 7.

A thin disc of copper, such as is used for engraving upon, about seven inches in diameter, was connected by wax with the glass plate employed in experiment 1, and the same apparatus was used with a needle of sealing-wax suspended above: on making the copper plate revolve rather quickly, the needle followed it, and went through two revolutions. Several plates of lead, pewter, brass and copper were tried, and the results were similar.

For the purpose of procuring more powerful and durable charges of electricity, I placed a small excited electrophorus on the three screws *a, a, a,* fig. 1, the copper plate was placed upon it. A very thin slip of glass *b, b,* formed a bridge, on the centre of which various needles were supported. The effects which resulted were not constantly the same, aad the unequal distribution of electricity on the electrophorus, combined with the action of the glass bridge, seemed to present very complicated results. I shall only detail one of these experiments.

Experiment 8.

The apparatus just referred to being employed, and the electrophorus without a copper plate being used, a needle of thin brass, similar in shape to that of experiment 1, with an agate cap, was supported by a fine needle point fixed by cement to the centre of the glass bridge. The brass needle rested nearly at right angles to the glass bridge (strictly at 98° from it).

On giving a slow motion to the electrophorus, the brass needle immediately advanced in the same direction about 10°, then stopped, then returned back to its original position, and, after a few vibrations, settled at a point 2° in advance of its original position. On increasing the speed of the electrophorus, the needle again advanced, and settled at between $3°\frac{1}{2}$ and 4° in advance of its first position. The velocity was again considerably increased, when the brass needle began to oscillate in a vertical direction, and the stationary point advanced to 9° or 10°, where it remained steadily, except that the vertical motion continued

The velocity was again increased, when the stationary

point moved on to 12° from its position at the beginning of the experiment. On stopping the motion, the needle advanced 115°, and then rested.

These experiments appear to me to admit of explanation, on precisely the same principles which I had previously applied to those relating to magnetism. It seems that electricity excited by induction is not *instantly* destroyed by removing the inducing body; nor is it instantly excited, to the full extent which the circumstances admit, on presentig an excited to an unexcited body. Admitting this principle, the whole of the reasoning which was given at the commencement of this paper, relative to rotations from magnetic causes, applies equally to those which arise from electricity.

The velocity with which electricity travels, has been urged as an objection to this explanation of the phenomena; and the immense rapidity with which the metals conduct that fluid, renders it difficult to admit that the motions employed in these experiments are at all comparable with that of electricity. There are two circumstances that may be stated as some explanation of this difficulty: the first is, that although the forces thus set in action are exceedingly weak, yet, from the nature of the apparatus, they act constantly for a considerable time, and in the same direction; thus gaining, by the duration of their total action, a power far superior to the almost infinitely small one which arises from the transient action of each part: the second is, that although the velocity with which electricity of considerable intensity moves through good conductors is excessively great, I am not aware of any experiments which prove that the small inequalities which are produced in a very weakly electrified body by induction from

some neighbouring body, are removed with the same velocity. The analogy of a fluid similarly circumstanced, would lead us to infer, that when the deviations above the level are minute, the return to it will be more slow.

Of Screens.

Although the preceding experiments, when repeated under various circumstances, had convinced me that the rotations which had been observed were not attributable to currents of air set in motion by the apparatus employed, I was desirous of examining what effect would result from the interposition of a screen. I therefore suspended the needles I employed by a silver wire, $\frac{1}{500}$ inch in diameter, and about two feet in length, which was contained in a glass tube, supported over the middle of the apparatus represented in fig. 1. This tube and wire passed through a circular hole in the centre of the glass plate R. The bridge *b b*, was removed, and a screen of fine muslin was stretched across the bottom of the lower cylinder Q, by means of the external one *c c*. This screen and the needles were occasionally changed.

Experiment 9.

A brass needle, having a very fine thread of spun glass cemented to one end by a little wax, was suspended by the silver, about $\frac{1}{8}$ inch above a screen of coarse gauze (not silk). A pewter disc, of 4 inches in diameter, was placed on the revolving stand, $\frac{3}{16}$ inch below the gauze screen. Neither the needle nor the disc were electrified; and they were completely protected from any currents of air moving about in the room in which these experiments were made.

The three first oscillations of the needle were:

1st. 127.8
2nd. 126
3rd. 127.8

The disc was then set in motion, and kept revolving at about 78 turns in a minute. After six minutes no result was apparent in the needle different from those which had been observed before the motion commenced. This seems to show that the action of air, if it is driven through the screen, is insensible.

Experiment 10.

Having placed a screen of coarse gauze above the pewter plate at $\frac{1}{8}$ inch distant, an excited needle of sealing-wax was substituted for that of brass. I observed the extent of the oscillations, and then setting the plate of pewter in motion, I was surprised to find that the returning arc (contrary to the direction of the plate's motion) was almost invariably increased, generally at the first, and almost constantly at the second oscillation, and that the arc in the same direction was diminished.

So unexpected a result naturally induced me at first to attribute it to some error in the observations; but on repeating it many times, and having measured the greatest error which the means I employed were susceptible of when used even a little carelessly, I found that half a degree was the greatest error I could make in reading off the oscillations, and that this retrograde motion was in many cases equal to three,

four, or five degrees; so that the measure of the observed fact was from four to eight times the extent of the greatest possible error of observation, even admitting it to take place at both extremities. It is true, I did not always observe this retrograde motion; but in twenty experiments the needle, screen and plate, not being always at the same relative distances, I noticed it seventeen times. The next step seemed to be to find some mode of encreasing the unequal distribution of electricity on the metallic plate, supposing that to be the cause of the phenomena. I therefore made a very small lamp, which being lighted, was placed on the stage L of the revolving apparatus, immediately under the copper. It was afterwards found convenient to make a small aperture in the lower millboard cylinder, just opposite the platform supporting the lamp, for the purpose of lighting it when requisite: this opening was closed by a piece of mica, fastened on a card.

Experiment 11.

A small stick of sealing-wax being suspended from the silver wire above a leno muslin screen, the pewter plate was supported just above the lamp, and under the screen, the following observations were made:

Oscillations of the needle.	Diff.	$\frac{1}{2}$ Diff.	Mean point.
°	°	°	°
R .... 16 L 18	2	1	17
The lamp was now lighted.			
R .... 15.5 L 25	19.5	4.75	20.25
R .... 18 L 31	13	6.5	24.5
R .... 14 L 34	20	10	24
R .... 14.25 L 33.2	19	9.5	23.7
R .... 16 L 28	12	6	22
R .... 21 L 25	4	2	23
R .... 20 L 29	9	4.5	24.5

The wax now became more heated, and gradually bent until the glass index touched the graduations. The number of revolutions of the pewter plate and lamp were 56 per minute.

Experiment 12.

The same apparatus being used, a brass needle was substituted for that of sealing-wax; it was suspended $\frac{1}{2}$ inch above the leno muslin screen, which was $\frac{1}{8}$ inch above the pewter plate; these distances were the same in Experiment 11. The plate and lamp made 56 revolutions per minute.

The observations were made as in the last experiment. The apparatus being at rest, the mean point was 2°.

Oscillations.	Diff.	$\frac{1}{2}$ Diff.	Mean point.
0	0	0	0
R.....0 L.....4	4	2	2
Lighted lamp, and set it and plate revolving.			
R — .5 L + 10.5	11	5.5	5
R — 1.5 L + 12	13.5	6.75	5.25
R — 8 L + 10.5	18.5	9.25	1.25
R — 5.8 L + 4	9.8	4.9	—0.9
R — 3 L + 6.5	9.5	4.75	+ 1.75
R — 9.5 L + 9.5	19.0	9.5	0.
R — 13.5 L + 12	25.5	12.75	—0.75
R — 15 L + 17	32	16.0	+ 1.0
R — 14 L + 12	26	13.0	— 1.0
R — 11 L + 9	20	10	— 1.0

The lamp burned with a very dull flame, $\frac{3}{16}$ inch long, the top of the flame about $\frac{5}{8}$ inch below the pewter disc; it was lighted by a small stick, which burnt with a flame, and gave more heat than the lamp during the short time it was under the plate. This experiment lasted about twenty minutes.

Experiment 12.

The same brass needle being used in the same apparatus; the distances remaining as before, the lamp was trimmed, so as to give a much brighter flame.

Oscillations of needle.	Diff.	½ Diff.	Mean point.
°	°	°	°
L.....0 R.....2	2	1	+ 1

The lamp was now lighted, and set in motion.

Oscillations of needle.	Diff.	½ Diff.	Mean point.
L — 5 R + 2.5	7.5	3.75	— 1.25
L — 1 R + 6.5	7.5	3.75	+ 2.75
L — 3.5 R + 6.5	10.0	5.0	+ 1.5
L — 5.0 R + 7.0	12.0	6.0	+ 1
L — 4.5 R + 6.0	10.5	5.25	+ 0.75
L — 3.5 R + 9.5	13.0	6.5	+ 3.0
L — 5.4 R + 4.5	9.9	4.95	— .45
L — 4.5 R + 4.0	8.5	4.25	— .25
L — 2.0 R + 5.5	7.5	3.75	+ 1.75
L — 3.0 R + 7.0	10.0	5.0	+ 2.0
L — 4.0 R + 6.0	10.0	5.0	+ 1
L — 4.0 R + 6.0	10.0	5.0	+ 1
L — 3.5 R + 9.0	12.5	6.25	+ 2.75
L — 10 R + 11.5	21.5	10.75	+ .75
L — 6.5 R + 10.0	16.5	8.25	+ 1.75
L — 6.0 R + 5.0	11.0	5.5	— 0.5
L — 8.0 R + 15.0	23.0	11.5	+ 3.5
L — 14.5 R + 16.0	30.5	15.25	+ 0.75
L — 15.0 R + 17.5	32.5	16.25	+ 1.25
L — 19.5 R + 17.0	36.5	18.25	— 1.25

Soon after the commencement of the experiment a mist arose within the case, which continued about five or six minutes: this frequently happened in the other experiments. The duration of the observations was about twenty-five minutes in the two preceding experiments.

On examining the column of the mean point of the needle, I am not inclined to draw any inference respecting it: whatever action may have been exerted on it, does not appear to have produced a marked or decisive effect on that point. I was induced to insert them, from the circumstance of the remarkable and gradual increase of the arc of vibration in both instances. This will be observed on looking down the columns of half differences, which is, in fact, the amplitude of each oscillation about the mean point.

The observed retrograde movement in the eleventh experiment being so small, and yet exceeding many times the greatest possible error of observation, I was now desirous of enquiring whether, amongst the accidental circumstances to which the needle was exposed, there might not be found some one to whose influence this apparent anomaly might be ascribed. The number of causes to which it might be attributed was considerable; but I hoped by variations in the circumstances of the experiment gradually to eliminate them.

The following list comprised the greater part of them. The reverse motion might arise from, 1. Currents of air in the room; 2. From currents of air excited within the apparatus; 3. By air driven through the gauze screen; 4. By heated air rising from the metal plate heated by the lamp; 5. By the vibration excited in the apparatus by the motion of the jack; 6. By the torsion of the silver wire to which the

needles were attached; 7. By the electricity of the bridge; 8. By the bending of the wax needle.

The first of these causes, namely, the action of currents of air in the room in which these experiments were made, was obviated by the form of the apparatus in which the needle was afterwards inclosed, and care was taken to render all the parts of it as close as possible.

The second source of this retrograde motion might be supposed to arise from the movement impressed on the air within the lower cylinder of millboard being communicated through the screen, not by actually passing through its apertures, but by the vibrations it might impress on it. The fact of the direction of the motion in the lower cylinder being in the contrary direction to that of the needle in the upper one, was exceedingly unfavourable to this hypothesis.

Before I relate the various experiments which I made, with the view of successively placing each of these causes out of action, I think it may be necessary to detail a few additional experiments, in confirmation of the fact.

Experiment 13.

Using the same apparatus, I substituted a disc of copper, 5 inches in diameter, and $\frac{1}{16}$ inch thick, instead of that of pewter; the sealing-wax needle was suspended half an inch above the leno screen; the lamp was placed at one inch from the edge of the copper plate, towards its centre:—the flame was large and smoky.

Oscillations of needle.	Diff.	½ Diff.	Mean point.
°	°	°	
L ... 347.8			
R ... 343.6	4.2	2.1	345.7
L ... 347.0			
R ... 343.5	3.5	1.75	345.25
The lamp was now lighted.			
L ... 348			
R ... 344	4	2	346
L ... 339.5			
R ... 346	6.5	3.25	342.75
L ... 331.2			
R ... 337	5.8	2.9	334.1
L ... 330			
R ... 339	9	4.5	334.5
L ... 334			
R ... 337	3	1.5	335.5
L ... 330			
R ... 342.5	12.5	6.25	336.25
L ... 332.5			
R ... 356.5	24.0	12.0	344.5
L ... 341.4			
R ... 396	54.6	27.3	368.7

At one part of this experiment the needle became stationary, as if it had arrived at the end of its arc; it then went on again in the same direction as before. This stationary point should perhaps be considered as the extreme right of one vibration, and the extreme left of the next: I have noticed it more than once in these inquiries. The column of the mean point of the needle has, in this experiment, marked the same retrograde motion on the first application of heat, and the same motion in advance, when that heat has continued, as in former ones.

Experiment 14.

The same apparatus; the copper disc revolving at ⅜ inch

below leno screen; the lamp in the centre $\frac{1}{2}$ inch below the copper; the sealing-wax needle $\frac{7}{8}$ inch above screen. I shall, in this instance, only give the gradual progress of the mean position of the needle, as calculated from the observations.

Position of the mean point.

0
203
203.5
Lighted the lamp.
200
190.1
189.9
199.9
205.3
213.3
212.0
211.5
217.3
216.5
221.3
224.6

This experiment presents the same results as the preceding one; and shows, that the observed fact is not influenced by the eccentric position of the source of heat.

As the silver wire, from which the needles were suspended, had been unwound from a small cylinder, I thought it probable that it might be a little twisted; in which case it would, from the weight of the needle, untwist until counterbalanced by the elasticity of the wire. If, in these circumstances, heat were applied, it might happen that the elasticity of the wire would be altered, and consequently, from that circumstance alone, that the needle might change its position. If this were the case, on reversing the direction of rotation

of the plate, the torsion, which was previously supposed favourable for the reversed motion of the needle, would now act against it; I therefore made the following experiment.

Experiment 15.

The motion of the copper plate was reversed, and reading in the new direction, the following were the mean positions of the wax needle.

°
15.6
16.0
The lamp lighted.
15.0
Observation lost.
2.0
1.1
31.5
37.2
58.3
45.5
50.3

The greatest regression of the mean point of the needle was nearly 14°; so that if torsion had any influence on the former experiments, it was unfavourable to the production of the retrograde motion.

Experiment 16.

A screen of sheet zinc, about $\frac{1}{16}$ inch thick, was placed under a needle of sealing-wax, at about $\frac{7}{8}$ inch below it. The lamp was placed on the platform below, having its wick at the distance of about an inch from the zinc. The following observations were made.

Mean position of the needle.

°
249.0
249.25
Lamp lighted.
250.75
256.25
258.75
256.25
254.75
260.25
258.75
262.25
260.5
261.25
261.5
268.5

In this case, where a source of heat was revolving under a metal plate, the needle suspended above did not retrograde, but moved on in the same direction as the heat: the small comparative recession, at the sixth and seventh observation, not being of sufficient magnitude to be noticed, and it never having returned as far back as the point from which it started.

From this experiment it appears, that, whatever may be the cause of these movements, it does not arise from any vortex produced by the moving part of the apparatus, nor is it owing to the passage of any air through the screens.

In order to prove that no tremors arising from the moving power, and communicated through the solid parts of the apparatus, had produced these effects, the following experiment was made.

Experiment 17.

The apparatus being in precisely the same state as in the last experiment, the lamp was not lighted, and the following observations were registered.

Oscillation of wax needle.	Diff.	½ Diff.	Mean position.
o	o	o	
L ... 259 R ... 255	4	2	257
L ... 258.5 R ... 256	2.5	1.25	257.25
The apparatus was set in motion; the lamp revolving, but not being lighted.			
L ... 258.8 R ... 256	2.8	1.4	257.4
L ... 258.8 R ... 256.4	2.4	1.2	257.6
L ... 258.6 R ... 256.5	2.1	1.05	257.55
L ... 258.8 R ... 257.2	1.6	0.8	258
L ... 258.2 R ... 256.8	1.4	0.7	257.5
L ... 258.2			

This experiment clearly proves, both from the stationary position of the mean point, and from the decrease of the arc through which the needle oscillated, that no vibrations are communicated from the jack to the needle.

I now tried the effect of winding up the jack, and found that it did not increase the arc half a degree on each side, although it caused the needle and silver wire to oscillate slightly, as a pendulum.

Considering it possible, by means of an agate cap, to suspend the various needles employed in these experiments,

with sufficient delicacy to allow them to obey the minute forces under investigation, I made the following experiment.

Experiment 18.

A card of the same form and size as the brass needle described in Experiment 1, covered with sealing-wax, having an agate cap in its centre, was balanced on the point of a sewing needle, cemented to a glass bridge, which rested on the screen. The wax card was about 1.5 inches above the leno screen; under which, at the distance of .3 inch below, the copper plate was placed. The lamp was placed below the copper, at the distance of .75 inch from its edge. No motion was communicated to any part of the apparatus; the waxed card needle rested at 168°. The lamp was lighted, and at the end of 75″ the needle had moved 8°; it gradually advanced towards that part of the copper immediately above the lamp, and at the end of four minutes and a half became stationary at 87°.5, a point not precisely over the lamp, but a little on one side of it.

Experiment 19.

The same apparatus and relative distances being used, no motion was employed; a piece of red-hot iron, one inch long, by half an inch broad, was placed on the platform below the copper, instead of the lamp. The red-hot iron was placed under 210°, about one inch below the copper; and the waxed card needle rested at 143°. The experiment began at

$4^h\ 23^m$
4 28 no effect.
4 32

The nearest end of the needle had moved over 47°, to a spot

not quite above the edge of the hot iron, and about 10° from its centre.

In order to get rid of the electrical effect of the glass or wooden supports, which I have called bridges, and which sometimes supported the needles, I placed a cork on the centre of the screen, immediately above the centre of the copper disc; into this was fixed the point of a sewing needle, on which the agate caps of the needles rested. I conceived this arrangement necessary, because, when no screens were employed, and the distance between the needle and the revolving plate was considerable, it sometimes happened that the needle followed the copper in its motion until it came over the supporting bridge, where it sometimes rested, instead of continuing its revolutions as long as the copper plate was kept in motion.

Experiment 20.

Using a wax card needle supported by a cork placed over the centre of the leno screen, and the lamp placed in the centre, under the copper plate, a slow motion was given to the lamp and plate. After some minutes, the needle had receded in a direction contrary to that of the motion of the plate through 35°. This experiment was repeated several times, and I observed that the needle, (if it moved at all,) generally went in an opposite direction to the plate; and that its motion was often so slow as to be imperceptible to the eye, although it could be very distinctly perceived after the lapse of a short time, by its altered position. In various repetitions of this experiment, the needle slowly retrograded from 10 to 90 degrees.*

* In one experiment which lasted above half an hour, and in which I had the advantage of Mr. Christie's assistance, the needle slowly retrograded over 157°. In this case the motion was rather slower than that of the minute hand of a watch.

For the purpose of determining whether the air, heated by the upper surface of the metal disc, acted on the needles, as it does on the vane of a smoke-jack, I made the following experiment.

Experiment 21.

Having formed a needle of sheet brass, thinner and lighter than those hitherto employed, but of the same form, I bent the circular parts, at the extremities, in such a manner, that they formed angles with the longitudinal axis of the needle; one of about 5°, the other about 15°. This being balanced above the copper plate, the lamp was lighted below it, but no rotation was communicated to the apparatus. The needle soon began to acquire vibrations in a vertical plane; but after five minutes' exposure there was no perceptible rotation. I now bent the circular extremities of the needle so as to form angles of about 35° and 40°. On replacing it, and lighting the lamp, the needle moved about 23°, in a contrary direction to that which the impinging air ought to have produced; it then returned, and went on through several revolutions in the opposite direction. The needle employed in this experiment projected a little beyond the copper plate, and would therefore receive some of the heated air passing from the lamp along the under side of the plate, to which cause the vertical vibrations may perhaps be ascribed.

From this experiment it appears, that if the needles are made flat with ordinary care, rotation will not ensue from the action of the heated air.

I shall now briefly restate the various causes to which these rotations might be ascribed, and refer back to the experiments which refute each supposition.

1st. The various currents of air in the room could not produce an almost uniform result. Considerable care was taken not to move about more than was requisite, and the experiments were generally made after the apparatus had been left, and the room shut up for some hours previous. In all but the earliest experiments the cylindrical millboard cases, in which the needles were placed, effectually excluded all currents of air exterior to them.

2nd. The rotation of a current of air produced within the lower cylinder by the motion of the lamp, metal plate, and the wooden platform supporting them, would, if it produced any effect, tend to make the needle rotate in the same direction. But we have seen from many experiments, that when a muslin screen is interposed, the rotation at the commencement is in the opposite direction. Also in Experiments 18 and 19 there was no rotation given to the apparatus, and yet the needle advanced; and in Experiment 16 a sheet of zinc prevented the air set in motion in the lower cylinder from communicating with that in the upper.

3rd. That air driven through the screen was not the cause of these motions, appears from their taking place when the screen was impenetrable to it. In Experiment 9, where no electricity was employed, a rapid rotation was kept up during six minutes immediately under a gauze screen, and no motion was produced in the needle above it.

4th. Perhaps the most probable of these suppositions is, that the motions result from the effect of currents of air which rise from the surface of the heated metal disc: these will cause other lateral currents, arising from the cold air flowing in to supply the place of that which has been heated,

and has ascended. Such an hypothesis might explain Experiments 18 and 19, if, on measuring its influence, the cause were found sufficiently powerful; but it appears from Experiment 21, that when the needles are not visibly bent in the form of vanes, this cause is not sufficient to produce the effect, although it acts in the same direction. The heated air above a revolving plate will revolve in the same direction; and if it pass through the interstices of the muslin, it ought to impress motion in the same direction as the plate from which it rises. The heated air above the zinc screen had not this rotatory motion; and we have seen by experiment 21, that its vertical action was not sufficient to produce the effect.

In many of the experiments no heat was employed; and yet the same motions resulted, as is apparent by Experiments 5 and 6 for direct motions, and by Experiment 10 for the retrograde.

5th. Vibrations excited in the apparatus by the motion of the jack. This cause was decisively refuted by Experiment 17, in which they were measured, and found to be quite insensible. Also in Experiments 18 and 19 the apparatus was not in motion, yet the needles did move.

6th. The torsion of the silver wire to which the needles were attached. Until I had measured this torsion by reversing the direction of the moving plate, as in Experiment 15, I had some suspicion that the heat of the lamp might have altered the elasticity of the wire; and although I did not then see how the reverse motion could be well accounted for, yet I was desirous, if possible, of removing altogether this disturbing action. Some of the previous experiments in

which no heat was employed, and many of the subsequent ones in which no wire was used, effectually refute this supposition.

7th. The electricity of the bridge might be supposed to produce some of the motions.

It was more necessary to get rid of the action of this cause than of any of the others, because electricity, modified indeed by the circumstances under which it was placed, did, in my opinion, produce the whole of these curious phenomena. The action of the glass or wooden support was apparent, and is noticed in the remarks on the 19th Experiment; but as it was not employed in any of those experiments in which the balance of torsion was used, and as it was dismissed from several of the latter ones in which reversed motions of the needle appeared, they cannot be attributed to this cause.

8th. The bending of the wax needle.

The application of heat after some time altered the form of the wax needle; it sometimes became so soft as to bend by its own weight down to the graduated circle. This change, however, only took place at the latter part of the experiment in which it occurred, and the phenomena recorded usually appeared at a much earlier period, when if it had commenced the change was quite imperceptible. In those experiments in which heat was not employed this cause was absent; and in many of those in which it was applied, a needle of card, or of thin brass covered with sealing-wax, or a mixture of resin and shell lac, was used, in which no bending took place.

Having shown that none of the causes to which I have

alluded are common to all the experiments in which the phenomena occurred, I shall now offer that explanation of them which appears to me to be warranted by the observations that have been recorded in the preceding part of this paper. They naturally divide themselves into two classes. 1st. Those in which the motion communicated is in the same direction as that of the revolving plate. 2ndly. Those in which the motion of the needle is in a contrary direction to that of the plate.

The instances in which the needles employed followed the directions of the revolving plate are so numerous, and the extent of their motion so great, that no doubt can remain of the fact, and some of them may be repeated with a very simple apparatus. The circumstance of one of the bodies employed being generally an electric, such as wax, resin, glass, and of the increase of the observed motions when these were previously excited by friction, or by a change of temperature during the experiment, naturally points out electricity as the cause. It is indeed the only one common to all the cases detailed.

If it be admitted that induced electricity is neither acquired nor given up in an instant of time, then it necessarily follows, from the reasoning I have explained at the commencement of this paper, that such motions must arise.

2nd. The case of a retrograde motion is much more difficult of explanation. Its existence rests on measures of much smaller magnitude, and it is by no means so easily re-produced, requiring some delicacy both in the adjustment of the apparatus, and in the observations of the needle. I have myself observed it so frequently, as to have not the smallest

doubt of the fact. I have occasionally shown it to several friends; although I have not in every instance succeeded in this.

To have omitted to state in this paper the fact of such a retrograde motion would have been uncandid, because it is one which is strongly opposed to all the reasoning that it contains. To abstain from giving any explanation, however imperfect, that might reconcile it with my view of the subject, would be to leave untouched a very powerful, and almost solitary argument against the explanation which I have proposed both of magnetic and electric rotation. I shall therefore attempt to show, not merely that it is not repugnant to the principle which I have already explained, but that that very principle may, in certain circumstances, produce the retrograde motion, and that in others nearly similar, it shall not take place; thus not merely showing the possibility of the fact, but accounting for its apparently capricious nature.

In figure 2, N represents the end of an excited needle, situated above a metal plate C, and having a muslin screen G interposed between them. All three being at rest, the screen and the metal plate will become electric by induction, and one of the two following arrangements must take place: either the electricity developed on the screen and on the plate will be of the same species, in which case it will be contrary to that of the needle N; or the electricity on the screen and the metal plate will be of different kinds, in which case one of them must be of the same species, the other of a different one from that of the needle.

In the first case let B B represent the curve of the intensity of induced electricity on the muslin screen, that is, let each of its ordinates be supposed proportional to the electricity at

the corresponding point on the screen; then, part of the influence of the electrified body N passing through the interstices of the screen, produces on the metal plate the same kind of electricity as it did on the screen. Let the curve of electric intensity in the metal plate be A. Whilst the metal plate C remains at rest, both its electricity and that of the screen tend to draw the needle N directly towards it; but if the plate C move in the direction of the arrow with an uniform velocity, and if induced electricity be not instantly annihilated, then there will arise a new electric equilibrium, and the curve representing its intensity and position will have advanced towards that part to which the motion is directed, and may be represented by the dotted line *a*. The influence of this electricity, which is of the same kind with that on the screen, and which consequently it repels, will be, that the electric curve on G must be driven back in the opposite direction: let it be represented by *b*, *b*. Now the attraction of *b*, *b* on the needle N tends to give it a motion in the direction contrary to that which is impressed on it by the attraction of *a*, which is on the opposite side of the perpendicular, dropped from N to the metal plate. That proportion of the resolved action of *b*, *b*, which communicates horizontal movement, is larger than it is at the greater distance of the plate C, and the whole force acts at a shorter distance: it may therefore exert a more powerful influence in turning the needle in that direction, than a stronger force at a greater distance, C acting more unfavourably. Thus the needle may, by the advance of the plate C, be caused to retrograde. It does not, however, follow, that this must always happen, for the susceptibility for induced magnetism in the plate may be much

greater, and it may retain it much longer than the screen, and in this case we ought to anticipate a motion of the needle in the same direction as the plate.

Let us now examine the case of one of the bodies possessing the same kind of electricity as the needle. Let the same letters in figure 2 represent the electricities when the plate is at rest; thus, when it moves in the direction of the arrow, the electric curve will assume the position of the dotted line *a*; and since it is of an opposite kind to that of the screen, it will attract it, and will tend to draw the electric curve B on the screen into the position *b*, *b*, marked by the dotted lines. In this case the electric forces are both on the same side of the perpendicular from the needle; but since they are of opposite kinds, one will attract, the other will repel the needle; and according to the strength of these forces, it will advance, be stationary, or recede. This explanation accords with the apparently capricious nature of the fact; the circumstances on which a direct or a retrograde motion depends are so numerous, and in the present state of our knowledge we have so few data for calculating their influence, that it is not surprising that the result of any given combination should be uncertain. Besides the relative distances, we ought to be acquainted with the relative conducting powers of the screen and plate,—the intensity which can be excited in each by a given inducing source—the quantity of that inducing action which can be transmitted through the interstices of the screen—the time each body takes to acquire and give up electricity—before we can attempt accurately to predict the result of any proposed arrangement.

There is another circumstance in which the above expla-

nation agrees well with the observed facts; since the motion communicated to the needle is the difference of two forces, each of which is very small, it must itself be smaller than either; and in case of any approach to equality in those opposing forces, it will become exceedingly minute.

With respect to the action of heat in encreasing these rotations, I am inclined to conjecture, that the distribution of electricity on the metallic plate is altered by the application of heat; that the part under the needle becoming electrified, produces in it induced electricity; and thus the plate being made to revolve, the rotation of the needle follows from the explanation previously given. It may, however, happen, that the wax needle becomes itself electric by the heat it receives from the plate below it.

It will naturally be enquired, whether a parallel to this retrograde motion cannot be produced with magnets. I have made a few trials with a plate of soft iron, an iron gauze screen, and neutralized magnets, but have not succeeded. I found the needles stationary over whatever point they were placed.

I shall conclude this paper with mentioning some of the methods by which I observed the angles passed over by the needles; this is the more necessary, because it was desirable to remove all the parts of the apparatus as far from them as could be conveniently done.

If the angle was small, a fine thread of glass was made to project from the end of the needle, and a lamp was placed in such a position as to throw its shadow on the graduations below; this of course would only do for the measure of a few degrees, and was inconvenient by day.

In some instances I used a plano-convex lens cut into halves, one half being fixed as much before the other as the distance of the needle from the graduations required; this brought the image of the glass thread and the graduations into the same plane; but as the lens I had cut was of too short focal length, this plan was not often adopted.

I occasionally employed a narrow ring of glass, on one half of which the graduated card circle was pasted; the back of the glass ring was blackened. By placing the eye above the needle in such a position that the direct image of the glass thread covered that which was reflected from the black glass, all parallax was avoided; and I found that, without great carelessness, repeated observations never differed more than a quarter of a degree.

Another method which I made use of was, to draw a fine line along the needle; and having made a small hole in the centre of the glass plate which covered the apparatus, I fixed an index of card, which could revolve about it as a centre; this index extended to a graduated circle fixed on the same glass cover. When I wished to observe the position of the needle, I brought one edge of the index to coincide with the line on the needle. There were some advantages in this method, but it had the inconvenience of obliging me to touch the apparatus at each observation.